DISCUSSION BEFORE THE WIRELESS SECTION, 10TH APRIL, 1929.

**Dr. R. L. Smith-Rose:** In connection with the direction-finding system illustrated in Fig. 2, the essential requirement is to prevent any reception due to the horizontal component of the electric force. To attain this end the author buries the horizontal conductors underground to a depth of 1 ft. and also shields them with an outer metallic covering which is well earthed at the ends nearest each vertical aerial. It is doubtful whether the burying of the conductor to such a small depth as 1 ft. makes much difference when working on wave-lengths of the order of 20 m, since the electric force above the surface must be the same as that just below, and it is necessary to go down an appreciable fraction of a wave-length before any detectable difference will be observed. The author is quite aware of this, because there would be no point in putting another conductor around it if it were already screened by the ground. One of the difficulties in arranging this is to test whether the horizontal conductor picks up any electric force, and that is very difficult to do. One might suggest disconnecting the vertical aerial at one point and testing whether any signals are being received, but immediately one connects up that vertical aerial the conditions are altered. It is the effect of the current in the outer horizontal screens on the vertical aerials which is the important point. As regards the results obtained with Adcock direction-finders, I am afraid that the author has spoilt the claim made in the paper by Mr. Barfield and myself, viz. that they do give a system free from night errors. On these short wave-lengths it is obvious that the direction-finder as a navigational instrument is useless in the presence of these night errors. In connection with Fig. 8, which shows the aerial arrangement with a switching system for altering the reception diagram from the horizontal to the vertical plane, it occurred to me that the possibilities of this system might be extended still further by making the phasing unit at the centre variable. If that could be controlled from the ground one could adjust until one received the maximum intensity and so employ this as a sort of direction-finder for reception in the vertical plane. The discussion of dispersion on pages 110 and 111 is somewhat difficult to follow on account of the question of group velocity. In the course of his analysis the author draws a distinction between groups of waves comprised of single frequencies, and, while I think I can appreciate what he means, I should like to ask whether he is quite right in using the term " single frequency." There is, I think, no such thing as a single frequency or mono-rhythmic radiation. What we term a single frequency is really a band of oscillations, and we cannot discriminate between the different frequencies within the band; and what we normally call a continuous wave is really a group of waves of that nature. The term " single frequency " should, I suggest, be used with caution. This is a rather important point, because it does enter largely into the group-velocity analysis described in that portion of the paper. On page 101 there appears to be some foundation for the idea that the waves travel round the earth's surface by a series of hops. I was always at a loss to understand why the waves decided to come out at a particular point; but this idea that they come down to earth periodically is, I think, a much more reasonable one, and I am glad to see that the conception is satisfactorily established at last. Fig. 24 is very interesting as affording definite evidence for the fading of signals due to rotation of the plane of polarization. Many investigators have had the idea that all fading must be definitely an interference effect, which implies two or three sets of waves travelling with different phases. The phenomena of fading can, however, be explained with only one set of waves, the plane of polarization of which is rotating. As regards " whistlers," I gather that they are due to a stream of particles coming from the sun, striking the atmosphere, getting stopped and setting up an electromagnetic impulse. That impulse is assumed to split up into a number of different frequencies which are propagated with different velocities. It has occurred to me that one way of investigating these whistlers would be to record them on a series of receivers adjusted to different wave-lengths, thus obtaining simultaneous frequency records over a range of wave-lengths covering a wide portion of the spectrum. By taking accurate time records one might detect the same impulse coming in at different frequencies all the way down the spectrum, and one could measure the time-lag and see whether it could be correlated with the audible effect due to the whistlers.

**Mr. J. E. Taylor:** In general I agree with the author's

conclusions. In this connection I was influenced by the measurements he has made with direction-finding apparatus showing a reflection back in the path of the beam, for instance, and giving an actual direction from the station towards which the beam is being sent, rather than from the station which is sending the beam. I agree with him that there can be no doubt that this indicates a scattering of the waves in the path of the beam. I was also influenced by the fact that, whereas for some time I was somewhat sceptical about the real existence of the skip distances, we have to admit that these have been fully confirmed and that skip distances for short waves do actually exist. Of course, there are still a large number of points on which we shall have to look for more explanation. One of these is in connection with the fact that these short waves have been found to traverse the circumference of the earth two or three times. Where one has waves completely encircling the globe in that way and travelling in both directions, as they may do under certain conditions, even though there may be night conditions on one side and day conditions on the other, one would expect to get, at the antipodal point to the transmitting station, places where the waves are concentrated. I have not yet heard of any actual reconcentrations of wave energy of that kind. Then, again, one wants a little more light, I think, on the very marked difference in range between short waves and long waves for the same power. Short waves, of course, have been proved to enable communication to be held over very long distances—the greatest distances obtainable on the earth—with a power much less than with long waves. What is happening to the energy in the case of these powerful stations using long waves? Clearly, the short waves must suffer a great loss of energy in their transmission through the atmosphere and over the surface of the ground. In agreeing with the author's views as regards the theory of these short waves, I wish to make one reservation. As a matter of fundamental principle, is a Heaviside layer actually necessary in order that waves may travel round the earth? In other words, if we have a spherical conductor—a metal conductor let us say—is there any scientific reason why the waves cannot travel round the conductor, whatever its size may be? Personally, I think that there is no good argument against it, and, fundamentally, waves will travel round conductors. Of course, the actual conditions obtaining on the earth, where we have not a metallic conductor nor a smooth surface, and have something conducting in the atmosphere, render the theory of wave transmission far from simple. The author has referred to " whistlers." It is now over 25 years since I paid some attention to these peculiar whistling sounds which can, I think, even more readily and clearly be observed with a single-wire telephone circuit earthed at both ends and situated in some remote locality where there is no interference due to artificial disturbance. Under such conditions they can be extremely clear. I have attributed them to meteoric matter entering the atmosphere, and although the author has given considerable attention to the subject, and advances some very good reasons for his view, I am still in difficulty on one point. If these whistlers are due to projectiles of any kind coming direct from the sun into the earth's atmosphere, why is it we hear so much more of them at night than in the day, seeing that at night the atmosphere is away from the sun from which the projectiles are supposed to be sent?

**Mr. R. A. Watson Watt:** The author refers to the fact that his conclusions rest very largely on the use of suitably designed radiogoniometers, and himself calls attention to some of the possible imperfections of the goniometers used. Dr. Smith-Rose has also referred to this point, but I should like to say that I feel that the author would have done well to adopt, in addition to the Adcock system (which was resuscitated and developed at the Radio Research Station), another aid to direction-finding which seems to me peculiarly suited to the purpose which he had in view. The use of a cathode-ray direction-finder, employing the Adcock antenna system, would have enabled him to follow visually the extraordinarily rapid and confusing variations in apparent direction of arrival occurring in the cases which he has studied. The author has referred to experiments by Friis in which use was made of the very valuable theorem that in a frequency-change system using a common beat-oscillator the relative phases of the high-frequency electromotive forces to be compared are conserved in the lower frequency. The ideal direction-finding system, for such short-wave research as that dealt with in the paper, would therefore appear to me to be an Adcock antenna system with frequency-change apparatus operating ultimately on a cathode-ray oscillograph as a visual direction-finder. The instantaneous response of this type of direction-finder is admirably suited for studying such rapid changes as made it necessary for the author to try to spin his goniometer search coil at high speeds; and, in the particular case of the mere delineation of the sector of scattering, the screen image on the oscillograph would give a direct delineation of this sector. It would, moreover, give a very valuable indication as to the extent of any failure to attain the ideal Adcock arrangement of complete insensitivity to the horizontal component of polarization. In dealing with the Friis experiment, the author assumes that he may infer from the visibility of a simple screen image that the propagation phenomenon giving rise to the image must have had a duration comparable with the period of persistence of vision. That this inference is not justified is sufficiently shown by oscillographic work on transients, and in particular on atmospherics, in which a clear visual impression is obtainable from a transient the total duration of which is of the order of a few tens or a few hundreds of micro-seconds. In amplification of the evidence as to the radio phenomena of the 10th October, 1928, I should like to add a report on the behaviour of long waves, the source of the data being one which bids fair soon to be the only surviving source of data on long-wave propagation. The curve of atmospheric disturbance, on a narrow frequency band centred on 13·5 kilocycles, on the day in question, had a form which we have learned to regard as typical of magnetic-storm conditions. At about 14 30 G.C.T. on this date a sudden increase in received atmospheric disturbance occurred within a period of 2 or 3 minutes, the amplitude of disturbance rising to between 2 and 3 times its value

before the change. Violent oscillation about the higher value persisted for approximately 1½ hours, the amplitude of disturbance falling to normal at about 16 00 G.C.T. The effect here was thus the more normal effect associated with magnetic storms, in which long-wave transmission is facilitated by improved reflection from the upper conducting layer. With regard to " whistlers," I think that the means of investigation which Dr. Smith-Rose has suggested is perfectly practicable but unnecessarily difficult. I believe that the author has in his own hands a much simpler method of observation, and I trust that he will try to obtain records of the actual frequency-changes, which I take to lie within the audio range, using the facsimile or picture receiver developed by his company. With such records before us we should have a much surer foundation for discussion on the mechanism of the " whistler."

**Dr. J. Hollingworth :** (*communicated*): This paper contains so much new and closely reasoned work that it is only possible in a discussion to raise isolated points, in which one may overlook their general application. In the first place, although it has not much application to the main body of the paper, a reference is made in the summary to the fact that my previous experiments on long waves give an over-estimate of the height. While this is probably true the error is probably less than 1 km, owing partly to the large angle of incidence used and partly to the very low ionic densities (100 to 200 electrons per $cm^3$) required for the purpose on long waves. In Table 1 it is certainly striking that the distance of the scattering source approximates in most cases to 2 000 km. At this distance, however, even a tangential ray will have reached a height of 300 km, which is considerably greater than the layer height of 100 to 125 km referred to elsewhere. Moreover, since this distance is constant it is presumably caused by some special ray for which scattering conditions are most favourable, and is not necessarily associated with the ray actually carrying the transmission to the distant receiver. In Table 1 the transmitters all appear to be comparatively close to the point of observation, giving a very small base for the triangle; have any results been obtained on stations slightly more distant, though of course within the skip distance? It is impossible to discuss the theory without very close study, although the case against the ray theory on long waves seems to be strong; and the whole emphasizes how little is really definitely known yet on the subject of wave propagation.

**Mr. T. L. Eckersley** (*in reply*): Dr. Smith-Rose has raised the question of the elimination of the effect of the horizontal e.m.f. in the type of direction-finding receiver described in the paper. The arrangement used is, of course, not perfect, but in practice the errors introduced by the horizontal component cannot in general be large. As an example we can compare the Canadian beam signals received on a frame and on the Adcock type of aerial. Whereas in the former case no reliable bearing can be obtained at all (the wandering and the flattening of the minimum being excessive owing to the effect of the horizontal component), when using the Adcock aerial the wandering of the minimum is usually less than about 2°. Exceptional cases do occur where the effect of the horizontal component is noticeable. Such cases usually occur near the edge of the skip distance, where the horizontal forces may be momentarily very large compared with the vertical forces. Considerable minimum wandering may occur at such times.

The following considerations give a certain amount of justification for burying the cable even to the depth of only 1 ft. Taking the conductivity of the earth as $\frac{1}{2} \times 10^{-12}$ C.G.S. units (at Broomfield, Chelmsford) as determined by various measurements, then for a 15-m wave the horizontal e.m.f. is reduced in the ratio nearly $e^{-1}$ at 1 ft. depth. At 3 ft. depth the shielding by the earth should be nearly complete. We propose to carry out tests to see whether such an increase in depth results in a marked improvement. Again, as we have found, and as explained by Dr. Smith-Rose, the effect of the horizontal field is indirect. It produces currents in the outer sheath of the cable, and the potential at the end of the cable sheath induces spurious e.m.f.'s in the vertical aerials. The effect of the burial of the cable sheath in the earth is to damp such oscillations very largely and so reduce their indirect action on the vertical aerials. Similarly, a direct connection to the earth at a considerable depth at the ends of the cables by a low-impedance conductor reduces the end potentials of the sheath, and I consider that such measures, taken all together, reduce the effect of the horizontal field to negligible proportions in all but exceptional cases.

With regard to the use of the two phased vertical aerials as a direction-finder in the vertical plane, I am of the opinion that this arrangement would be too difficult to handle and the results obtained too difficult to interpret for it to serve any useful purpose. For one thing, it would be very difficult to measure the exact misphasing between the two halves of the vertical aerial; and for another, the directional characteristics of the aerial in the vertical plane are not sufficiently well defined to obtain any very definite results.

With regard to dispersion, I take it that Dr. Smith-Rose is criticizing something I may have inadvertently said, rather than the text of the paper. In the discussion of dispersion on pages 110 and 111 the whole argument is based on the assumption that a " dot " is composed of a narrow band of frequencies, and nowhere have I even hinted that a limited dot can be considered to be a monochromatic radiation. Thus on page 110 I state " A group of waves, constituting a dot, contains a narrow band of frequencies, each of which travels with its appropriate phase velocity. . . ." Later, " It (the dot) really contains all frequencies from 0 to ∞." It is implied in the argument that the form of the group constituting a dot depends on the fate of the various frequencies which compose it, each travelling with a different phase velocity, and therefore it is both explicitly stated and implicitly implied that a dot is composed of a spectrum of frequencies.

The exact mode of propagation of short waves over long distances has always been a puzzle. Although evidence is cited in the paper that long-distance transmission occurs by multiple reflection between the earth and the Heaviside layer, it is not sufficient definitely to exclude other possibilities. Recently, in examining

the multiple signals recorded in the facsimile transmission, I have obtained evidence, which seems to me much more definite, that transmission takes place by multiple reflections, but I am not yet convinced that this is always the case.

The method suggested by Dr. Smith-Rose of obtaining the frequency/time curve of the whistlers has occurred to me but has not yet been put into practice. The apparatus required would be rather complex and I have a great aversion to complexity.

Mr. Taylor has raised the question of reconcentration of echoes at the antipodes and near the starting point of the source of waves. Without measurements (and as far as I know none exist) it is difficult to know whether such reconcentrations exist, but estimating by ear, on signals reconcentrated near the transmitter, I should say that there is no startling evidence of such an effect. It must be realized, however, that such a reconcentration requires an extraordinary constancy of the electrical length of the various paths reconverging at the antipodal point, which in view of the variability of the upper atmosphere is not likely to exist.

The difference in range between short and long waves of the same power is, I think, more apparent than real. If we compare a short-wave and a long-wave station on the basis of the same radiated power the difference does not appear to be so great. Thus, compare a long-wave and a short-wave station transmitting from New York to England. A station with 700 amperes on a 20 000-m wave, radiating from an aerial of 100-m effective height, will radiate about 20 kW and produce a field strength in England of about 100 $\mu$V/m. A 15-m station in New York radiating about 10 to 15 kW will produce an e.m.f. of about 30 $\mu$V/m. The disparity is not great, and is rather in favour of the long-wave station.

I am afraid that nothing will shake my firm belief that the Heaviside layer is necessary for the transmission of electric waves round the earth. I consider that this has been proved mathematically by Poincaré, Macdonald, Nicholson and Watson, and is supported by observation.

Whistlers form a fascinating study. Some while ago Mr. Tremellen and I discussed the possibility that they were due to meteorites, but we could find no physical reason for the electrical effects produced. In particular, the association of whistlers with magnetic storms and the definite echoes heard finds no adequate explanation on the meteorite theory. The difficulty experienced by Mr. Taylor can be easily explained. The streams of particles which are emitted by the sun are sufficiently charged in the neighbourhood of the earth to experience a large deflection, on account of the earth's magnetic field. The effect of this, as Prof. Störmer has shown, is to twist the streams into the polar regions and in many cases to precipitate them on the darkened half of the earth. Also I picture whistler disturbances to be of almost world-wide extent, so that even those originating in the sunlit hemisphere would spread to the night regions.

I agree with Mr. Watson Watt that the combination of an Adcock aerial with a cathode-ray oscillograph indicator would have been specially suitable for our purpose. In view, however, of the relative complexity of such a system it seemed better to exhaust the possibilities of the simple system first. We thought we could get the maximum information in a given time by using the simple system and we are now preparing to use the oscillographic method. In discussing the Friis experiment I was using my own experience of the difficulty in getting transients of very short duration. As with better technique I understand that Mr. Watson Watt is able to observe these short transients, the argument used by me breaks down. The observation of atmospherics on the 10th October is very interesting, particularly in respect of the fact that the sudden change in atmospheric activity occurred at a different time (14 30 G.M.T.) to that of the short- and long-wave fade, which occurred between 11 00 and 13 00 G.M.T. and was characteristic of *normal* magnetic storm conditions, although no magnetic storm ($\gamma > 100$) was reported on this date. Such a result is difficult to explain on any theory. It might possibly be surmised in accordance with the suggestion made in the paper that the deeply penetrating disturbance starting at about 11 00 G.M.T. and lasting for an hour or so, ionized the whole layer down to a much greater depth than usual after the renewal of the ionizing agent at about 12 30 to 13 00 G.M.T., and that the ions in the lowest layers rapidly recombined, leaving the normal layer more highly conducting than usual at 14 30 G.M.T., but this hardly accounts for the rapid initial increase of long-wave X's at this time.

Dr. Hollingworth has referred to his measurement of the effective height of the long-wave layer in daytime by interference methods. In the short-wave range this, as we know, gives the apex of the equivalent triangle made by the ray, and is therefore an over-estimate of the height. But in the long-wave case the considerations given on page 132 show that in the medium the wave loses its ray formation, and it is therefore difficult to use the former interpretation. In fact it is difficult to give a precise meaning to the equivalent effective height as measured on these waves. Although in the absence of attenuation and the earth's magnetic field the waves cannot penetrate to regions where the electron density is greater than 200 to 300 per cm$^3$, this is not the case in practice, where attenuation and the earth's magnetic field have to be taken into account and the usual ray interpretation of the effective layer height cannot be used. I therefore think that it is difficult to state how near the effective height derived from the measurements is to $h_\lambda$, this being defined as the height where $N_\lambda$ is the critical density for the particular wave-length used. I should estimate this difference to be only a few km, but this estimate is only based on the particular model used and cannot be entirely justified. The distance of the scattering source found from triangulation is rather surprising, but is confirmed more or less accurately by time-delay measurements. The bases of the triangles are rather small, but unfortunately with the stations situated as they are now it is impossible to get greater bases without moving the receiver. In the present state of our knowledge I should not like to venture an interpretation of this distance, but it is perhaps significant that it is of the order of the skip distance of the majority of the stations observed.